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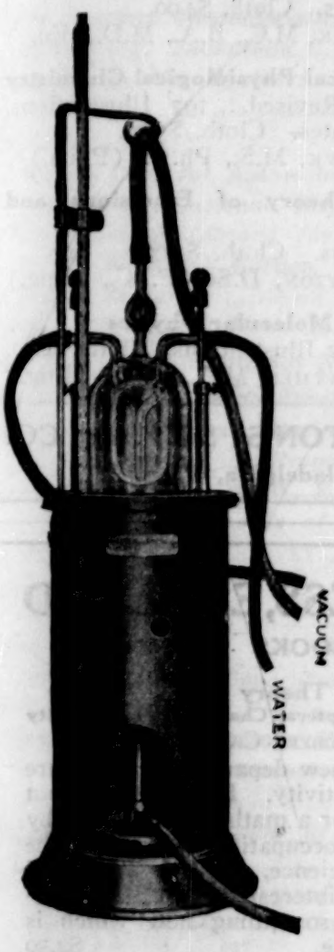

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THE METHODS OF IMMUNOLOGY[1]

DURING recent years your president has been denied the opportunity of continuing research work, by reason of constantly increasing administrative responsibility. Any one of you who has had similar experience will understand the sacrifice involved in being divorced from immediate participation in investigations. For the reason stated I am unable to present to you any piece of research, but in lieu of this I shall venture to tell you briefly of the great aid afforded by your own particular branch of biological science to those of us who are confronted with the responsibility for the final supervision of certain medicinal agents.

It is a fact that governments (and particularly our own) have singled out from the great list of remedial and prophylactic preparations those susceptible of examination by immunological methods, and have placed in the hands of certain agencies control procedures applicable to those agents and unthought of in connection with other groups of therapeutic preparations. Let me make this clear by an example:

At present no one may exploit in interstate traffic in the United States serums or vaccines for the treatment of cancer, but there is nothing to prevent any one from exploiting a treatment for cancer made of something other than a serum or vaccine, provided only that the labels on the preparation conform to the requirements of the food and drug enforcement officials.

In our country Congress in 1902 recognized the importance of, and the special dangers and needs in connection with, biologic products, and in the intervening period the hygienic laboratory of the United States Public Health Service has had exceptionally good opportunities of becoming familiar with the difficulties involved, as the control measures are exercised through that institution.

[1] Presidential address before The American Association of Immunologists, Boston, March 29 and 30, 1923.

I shall now briefly review the procedures in connection with the preparations in which biologic control is exercised.

SERUMS

We naturally consider first the *antitoxins* which have well-recognized potency standards. I shall dismiss diphtheria and tetanus antitoxins by saying that the standards for these are so satisfactory and so well known that the distribution by the central testing laboratory of standard serums for comparison enables producers to titrate their products satisfactorily, and, beyond occasional testing of market samples for comparison of results with those secured by manufacturers, no further measures are necessary.

It is less well known that there has been established a standard for botulinus antitoxin of types A and B, and a standard for the antitoxin against gas gangrene—*B. Welchii* or *B. perfringens*. In the case of these products, however, it is customary for manufacturers to control all of their own tests by forwarding samples to the central testing laboratory for the making of comparative determinations. We are indebted to Miss I. A. Bengston for the standard in the case of each of these anaerobes.

Finally, under this head it may be stated that we are endeavoring to establish an antitoxic unit for antidysenteric serum, but thus far the work has scarcely gone beyond the place where we can say that the serum has or has not antitoxic value. Perhaps if bacillary dysentery of the Shiga type were a more serious problem in the United States we might make better progress in this line.

Antibacterial serums: The most important of these is antimeningococcic serum, and it has occupied a large part of our time during the past five or six years. Starting with the simple agglutination test against the various types of meningococci we have gradually extended the work until at present, in addition to this, we often employ the complement fixation test, and even more often the determination of the bacterio-tropins; tests for the latter in this particular connection have been placed on a workable basis by Miss Alice Evans. The protection tests, devised by Gordon a few years ago, were given a fair trial but discontinued as we were unable to get such consistent results as Dr. Gordon seemed to have been able to secure. Indeed, in this case it is not at all clear that protection tests would be of more value than the tests we now employ, since it is possible that the mechanism of protection in animals might be entirely different from the mechanism of therapeutic action in the spinal canal, as the serum is ordinarily employed. Full recognition is of course given to the various types of the organism in all of our tests.

Antipneumococcic serum has been tested at an expense of time and money perhaps unwarranted by the practical utility of the preparation. A mouse protection test, modified from the one originally employed by Neufeld and later extensively used by the workers at the Rockefeller Institute, is in constant use and unquestionably gives a fair degree of evidence on which to judge of the value of the preparation, so far as can be determined by animal tests.

Antistreptococcic serum: In connection with this we have to record only failure to accomplish anything beyond tests which show streptococci have been employed in immunization of horses, and that the resulting serum is protective against small doses of the homologous organism.

It is an interesting fact that this serum, which is on such an unsatisfactory basis from a scientific point of view, is very extensively employed by clinicians, perhaps more extensively than any of the other antibacterial serums.

Antianthrax serum: This serum is not extensively employed in human medicine, but it has occupied the efforts of several of our workers and we are hopeful that satisfactory protection tests may be adopted in the near future. Our work so far has clearly shown the futility of strictly test-tube immunological examinations.

BACTERIAL VACCINES

In connection with this group of rather extensively used preparations, we are again confronted with the question as to whether the value of the preparations is such as to warrant the time and money spent on the efforts at standardization, beyond those necessary to establish tests to assure the safety of the product, since time and money are not available adequately to control all preparations coming under the law and regulations.

Another question, which may fairly be raised

in connection with preparations falling into this as well as into some other groups, is whether or not it is possible that the standardization of preparations of dubious prophylactic and therapeutic activity may give rise to a fictitious and wholly unwarranted confidence in the preparations. An exception must be made to this generalization, and this of course is with reference to antityphoid vaccine, the proven worth of which has made it necessary to adopt a standard test on its agglutinogenic qualities, a criterion which we realize is not satisfactory, although it is the best we have.

TOXINS AND SIMILAR PREPARATIONS

We consider here diphtheria toxin for the Schick test. Standardization is based on the Minimal Lethal Dose, and to meet the common practice of the day provides for dosages of one fiftieth and one fortieth of the guinea pig minimal lethal dose as the human dose.

Toxin-antitoxin mixture: The mixture is tested for toxicity, both as to its ability to cause death to the test animals within the usual period of four days and its ability to produce paralysis and later death.

POLLEN EXTRACTS

Pollen extracts are on a most unsatisfactory basis and we are now working on serological tests, controlled by clinical observation, in the hope that one may be adopted or developed which may serve as an index to the desensitizing value of the preparation.

TUBERCULIN

This is still another of the preparations which have occupied more of our time and thought than is warranted by their importance from a therapeutic or prophylactic point of view. We are unable to report any successful outcome of the work which has been devoted to this preparation.

VIRUSES

The most important of these is smallpox vaccine, and there is now actually in use at the hygienic laboratory a method (not original with us) which, by the inoculation of rabbits with a series of dilutions of commercial vaccine, enables us to form a rough estimate of the potency of the material. This particular preparation is so susceptible to external influences, particularly to the temperature at which it is kept, that a potency test satisfactory to-day gives no indication whatever of the efficiency of the material a few days later, if it has been kept under conditions unfavorable to the preservation of the virus.

ANTIRABIC VIRUS

The several modifications of the classical Pasteur treatment, which itself is still in use, have all been examined sufficiently to lead us to believe that the various preparations will, when properly used, serve to prevent the development of rabies when applied sufficiently early.

From this hasty survey you will see that the immunological art is very extensively applied in the control of biologic products and that there are ample fields of investigation in this line which, when sufficiently worked, will enable us to place all of the preparations which are of value on a sounder basis than we have for many of them at the present time.

G. W. McCoy

HYGIENIC LABORATORY,
U. S. HEALTH SERVICE

SOME MODERN PROBLEMS IN LEATHER CHEMISTRY[1,2]

WHEN the literature of leather chemistry of twenty-five years ago is compared with the publications of to-day, one is impressed by the extraordinary progress that has been made in a quarter of a century. The striking feature of the newer papers lies not so much in the results of technical and practical use, although such are to be noted, but first and foremost in the entirely different point of view in the choice and treatment of the problems. The pioneers in leather chemistry, among whom I would especially mention W. Eitner in this connection, have collectively produced a lot of valuable experimental data which they worked up solely from the standpoint of direct practical application. They have opened up an exceedingly fruitful field of experimentation, also quite naturally utilizing contemporary

[1] Translated from German by A. W. Thomas.

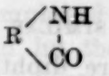
[2] Presented before the Leather Division at the sixty-fourth meeting of the American Chemical Society, Pittsburgh, Pennsylvania, September 4 to 9, 1922.

scientific knowledge. Activity in pure research, however, for the purpose of enlightenment and explanation, entirely unfettered by any thought of practical application, in which the complete structure of modern science is employed, has evidenced itself in the field of leather chemistry only within the past decade. One name joins these two phases of the development of leather chemistry like a bridge—H. R. Procter, who has given us in his "Principles of Leather Manufacture" a large number of very valuable conclusions based on experimental observations, and whose studies on the swelling of gelatin bear the character of the purest scientific research.

How the example set by Procter is being felt by the younger generation and how they are successfully progressing on the road of pure investigation is especially evident in the United States. Such a lively interest in purely scientific studies was unheard of in this field in the past century.

I may be permitted to touch upon a few of the problems which are considered of timely interest by modern scientific leather chemists and to contribute a few remarks to the discussion of these questions which will undoubtedly be brought out in their September, 1922, meeting. My selection in this connection is—our conceptions on the chemical nature of collagen, the mechanism of bating, and what we call the astringency of vegetable tannins.

Concerning the constitution of collagen we know merely what the study of hydrolysis has taught us. From hydrolysis we have a quite exact knowledge of the ultimate split products, the amino acids, but we are only very incompletely informed concerning the nature of their participation in the construction of the collagen molecule. We are indeed justified in assuming that in the main the amino acids are bound together in a peptid-like manner; but this leads us only so far as the peptones, *i. e.*, to those large building stones of which the collagen complex is constructed. We know practically nothing about the manner in which the peptones are bound together. It appears certain that their linkages are fundamentally different from those of the amino acids within the peptone molecule, since pure proteolytic enzymes which cannot effect any decomposition of peptide groups (*e. g.*, pepsin), can break proteins down only to the peptone stage. It is possible that the peptones are combined in a special fashion, which while resistant to peptolytic enzymes (*e. g.*, Erepsin) is vulnerable to the attack of proteolytic enzymes (pepsin). Up to date nothing definite is known concerning the nature of such combination. Plimmer's supposition that it is a question of anhydride-like groupings of amino acids is unproven, and the alternative conception of J. A. Wilson[3] that collagen and gelatin can be represented by the formulas



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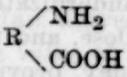

respectively, is not very convincing, since it is difficult to see why the -NH-CO- groups in collagen hydrolyzable by pepsin should not be attacked by trypsin, while in other respects the behavior of the peptid groups is just the reverse.

If the peptone linkage is assumed to be through primary valences, then we are forced to seize upon a kind of linkage entirely different from the peptide groupings, and concerning which, as previously mentioned, nothing is exactly known up to the present. It is possible, however, that the nature of the connections between peptones within the protein molecule is not at all through groupings which depend upon the saturation of primary valences, but that it consists of much looser bonds in which the peptones play an independent rôle. It is conceivable that this loose connection between the peptones becomes more tenuous during swelling of hide perhaps due to the enlargement of the watery envelopes existing around each single peptone molecule. Accordingly it is clear that swelling could be regarded as a preliminary stage in the peptonizing (*i. e.*, decomposition into peptones). It is also plain to be seen according to such a conception that the skin of young animals, rich in water, would be more easily transformed to gelatin than the hide of older animals, and further that the process of liming would become

[3] *J. Am. Leather Chem. Asso.*, (1917), 12, 108.

more difficult to carry out or even be frustrated in purpose upon dehydration (with alcohol) or by drastic desiccation. There are many examples illustrating the point that gel substances are the more readily peptized, the larger the watery envelopes about the single particles and thereby the greater the distance between them. In collagen the peptones may play the rôle of the individual particles, bound together by weaker valence forces than is wont in the case of primary valence. Just what this kind of affinity will be called remains to be seen. In any case, for the purpose of a preliminary working hypothesis, the following assumption may be of service: Peptones are bound to each other and to other smaller complexes in a loose manner, and are somewhat more strongly separated from other similar peptone complexes by larger water shells. During swelling of the protein these loose bonds can become still more tenuous.

The question now arises as to what influence swelling of collagen exerts on the action of enzymes, where the swelling is caused only by neutral salt solutions, and not by acids nor alkalies, in order to exclude the possibility of hydrolyzing action, *i. e.*, splitting of peptide linkages, by H^+ or OH^- ions. Experiments carried out in the Institute of Leather Chemistry at Darmstadt, and which will be reported elsewhere, have shown that the action of trypsin on collagen (white hide powder) depends to a high degree upon preliminary swelling in neutral salt solutions. The following results are especially noteworthy. If collagen is strongly swollen in normal KCNS or KI solutions, trypsin will almost completely dissolve it. It does not matter whether the trypsin has been added with the swelling salt or afterward. This tryptic action is predominately proteolytic, *i. e.*, it converts the protein predominately to peptones or peptone complexes and splits these into amino acids only to a relatively slight degree, which is shown by the fact that the hydrolysis products in the solution give copious precipitates with tannin, while formol titration yields smaller values than in the absence of the swelling salts. It appears then that the decomposition goes only to the peptone or peptone-complex stage and that the peptolytic action of the trypsin is prevented by the salt present. It is further worthy of note that very concentrated solutions of thiocyanates (5 to 8 N) and of iodides (3 N) have an inhibiting effect on the proteolytic as well as on the peptolytic action of the trypsin, and an astringent or shrinking action of the salt solutions runs parallel thereto.

Experiments with chlorates, nitrates, chlorides and sulfates have shown that these salts bring about swelling, decreasing in effect in the order named, and their influence on the action of trypsin is parallel to the decreasing swelling. Experiments were performed where pieces of pelt were treated with trypsin in N/2 solutions of the various salts at 37° C. The trypsin acted most vigorously upon the pelt, in the presence of thiocyanates, and the activities of the other salts followed at considerable intervals in the order iodides, chlorates and nitrates, chlorides and sulfates; the same order that obtained for the swelling power in the absence of trypsin. The effect is scarcely noticeable with chlorides, and in the case of sulfates it is doubtful whether the effect is null or even slightly negative, *i. e.*, whether sulfates exert a weakly inhibitory effect on the proteolytic action of trypsin.

I wish to especially emphasize the fact that the comparative experiments were all undertaken at the same hydrogen ion concentration (electrometrically measured). It is evident that it is not correct to explain the swelling power of an electrolyte solution solely and entirely as a function of the hydrogen ion concentration. The anion of the electrolyte exercises a very pronounced influence, and the Hofmeister series are not yet to be relegated to the class of abandoned fallacies.

What further conclusions may be drawn from the above observations?

First and foremost care must be exercised concerning the presence of salts in the bate liquor. The greater the swelling effect of these salts, then the more dangerous the activity of the trypsin, since it can lead to deep-seated peptization and to heavy damage to the hide.

Further, the various, widely differing observations which have been published concerning the effects of enzymes (especially trypsin) on hide can be understood. Different experimenters have obviously used collagen of very different degree of swelling, and consequently obtained discordant results. It is also under-

stood why collagen from different animal hides and tendons, while not showing any distinct analytical variations, have behaved differently in their ease or difficulty of conversion to gelatin.

It is already on record in physiological textbooks that collagen that has been previously treated with hydrochloric acid will be attacked by trypsin. That this effect is not a function of the hydrolyzing action of the acid, but rather due to the increase in the swelling is evident from the analogous action of neutral salts which bring about swelling of the collagen.

A new question which turns up is worthy of consideration, namely, whether the fixation of tannins is increased by the loosening of the peptones in the collagen-complex, that is, whether an increased development of surface is effected in the hide by the swelling with neutral salts, which would be shown by an increase in the amount of tannins capable of being fixed. Such experiments are contemplated.

I might now turn to another question which stands out prominently in the field of leather chemistry; the character of astringency. We combine with the idea of astringency the conception of tanning intensity, but until recently there has been no means at our disposal to determine comparative differences in degree of tanning action between two tanning agents which showed the same "tannin" content.

Wilson and Kern[4] have attempted to accomplish this through the determination of the tanning velocity, based on the assumption that a certain proportionality exists between astringency and speed of adsorption. Another method could be suggested from the viewpoint that a strongly astringent tannin will be taken up by a surface of weak adsorptive forces, while a tannin of low degree of astringency requires a powerfully adsorbing surface. A fractionation of the tanning material takes place according to the astringency in each liquor, since hide already considerably tanned and therefore of low adsorptive power will take up only the most astringent fractions in the strongest liquors, while fresh untanned hide will take up the mildest, least astringent tannins and even "non-tans." To test this view it is merely necessary to prepare hide powder of varying degrees of tannage, taking care that all soluble matters are washed out of the tanned powders. Treatment of different tannin solutions with these hide powders of varying degrees of tannage should yield information regarding the relationship of the astringent to the mildly acting parts present. Such experiments are now in progress in this laboratory and it will soon be seen whether they lead to useful conclusions. This procedure will by no means touch upon the question of the reason for astringency. Very interesting experiments by A. W. Thomas and S. B. Foster[5] were applied to the elucidation of this problem, and the results render it quite conceivable that astringency is a function of the electrical charge of the colloidal tannin particles. Previous to learning of this research I held the view that astringency was connected with the size of particles of the tannin. In order to test this assumption, it would be necessary to study the distribution of particles according to size in various vegetable tanning materials. Such an investigation has been pursued in this laboratory and will be published elsewhere. A few of the results are briefly as follows:

When a tannin solution is fractionally salted out, the first fraction will contain the largest particles, the later fractions, smaller particles, and in the final filtrate there will be the smallest non-precipitable tannin particles. When the separate precipitated fractions are filtered off, redissolved, and their amounts determined, comparable figures are arrived at which disclose the size relationships of the tannin particles in different tannin solutions. In this way large differences with various tanning materials are manifested. For example, an extract of quebracho wood prepared in the laboratory is rich in large and medium tannin particles and relatively poor in the smallest non-precipitable particles. Sumac is entirely different in that the greater part is composed of the smallest particles and very few large and medium particles appear to be pres-

[4] *J. Ind. Eng. Chem.*, 12, (1920), 465.

[5] *J. Ind. Eng. Chem.*, 14 (1922), 191.

ent. Thus there is a characteristic picture for each tanning material, that certainly is also influenced by the concentration and the manner of preparation of the respective solutions. The effect of age and non-tans on the size of particles is interesting. No two solutions behave alike upon aging. There are those which undergo a very considerable development in size of particles (*e. g.*, quebracho and mangrove) and others, in which this does not happen (*e. g.*, gallotannin, oak wood and chestnut wood). In any case one is not justified in assuming that no gradual change has taken place when a tan solution remains clear (no separation of ellagic acid or phlobaphenes). A solution may remain clear while undergoing an increase in size of particles which will be manifested in its tanning effect.

The effect of non-tans seems in most cases to be an increase in the degree of dispersion or in an inhibition of flocculation, or, in other words, diminution in particle size. This recalls the observation of Wilson and Kern where the speed of tanning was shown to be greatly reduced by gallic acid and suggests a relation between size of particles and speed of adsorption. All non-tans, however, do not behave alike; sumac non-tans, for example, have rather the opposite effect.

A closer examination of this non-tan question has shown that the sugar-like non-tans exert no influence on the size of the tannin particles, but that the phenol-like bodies (gallic acid type) have a pronounced effect. Gallic acid diminishes the precipitable fractions of chestnut tannin and of gallotannin to about one half. Here again exceptions are encountered, for quebracho and sumac are not markedly affected by gallic acid in respect to salting out.

This fractionation of tannin solutions is purely colloidal (according to particle size), and not purely chemical (according to components of different chemical constitution) as proven by the fact that the Lowenthal factor of all fractions is the same. Only in the case of oak bark, which is known to contain a mixture of tannins, one reacting green with ferric salts, the other blue, did fractional salting out effect a separation, although incomplete, of these components. This separation was evidenced by the regular change of the Lowenthal factors and of formaldehyde precipitation. In this treatment the tannin reacting green with ferric ion proved to be of higher dispersity than the blue-reacting tannin.

It yet remained to be tested whether the separate salted-out fractions showed any difference in their other colloidal properties. Viscosity and surface tension were measured with this object in view and compared with the corresponding values of the original tannin solutions. No direct relation between size of particles and viscosity was found, but the surface tension figures did prove to be dependent upon particle size. The first fractions (largest particles) always lowered the surface tension of water to the greatest extent, the decrement decreasing with each further fraction.

The increase in size of particles which ensues if the addition of sodium chloride is maintained under the critical precipitation concentration can likewise be explained as due to lowering of the surface tension. Further, aged tannin solutions also show increased lowering of the surface tension corresponding to increase in size of the particles, with the exception of quebracho. Conversely, there is a distinct correspondence with the fact that all processes or effects tending to decrease the size of particles have the expected effect on the surface tension. Among other things we might mention the diminution of size of particles through short heating of tannin solution, and the especially high degree of dispersion possessed by tannins set free from their lead salts by hydrogen sulfide.

This scanty summary of a few experimental results shows what a fruitful field for colloid-chemical result is offered by our vegetable tanning materials and it is to be expected that many points of practical application will be found when greater light is thrown upon the understanding of the problem of astringency.

The ultimate aim, however, in this and in all other leather chemistry researches is the widening and deepening of our fundamental knowledge of an as yet very undeveloped field.

ED. STIASNY
LEATHER CHEMISTRY RESEARCH INSTITUTE,
DARMSTADT, GERMANY

ON THE SPECTRAL CHARACTERISTICS OF PROTECTIVE COLORATION

THE following note is of the nature of a preliminary report on some investigations made last summer at Woods Hole. In this investigation an answer was sought to the following specific question: Is the color of a protectively colored animal spectrally similar or dissimilar to the color of the background which it matches?

The significance of the question lies in the light it can throw on the nature of color vision in animals. Take, for instance, the color of the frog—his head and back so closely resembling the slime of stagnant pools that it is difficult to detect him. If, in the first place, this color is adaptive, if the resemblance really helps to conceal the frog either from prey or enemies; and if in addition it can be shown that the frog's green and the green of the slime are, spectrally considered, different mixtures, it follows that the organisms from which the frog is concealed have color vision which in this respect at least agrees with our own. Color, as a natural fact, is not a physical but a psychological entity and a knowledge of facts like this is a knowledge of the *experience* of other organisms. There are those among biologists who deny the existence of adaptations in the sense of directed evolutionary responses by an organism to an environment. It is perhaps not out of place to note that such a denial is here beside the point. All that is really postulated in the case of the frog is that green frogs are safer from enemies or more dangerous to their prey than red frogs would be, so that while the language here implies a particular point of view the conclusions are largely independent of the biological philosophy which may have directed the work.

One obvious difficulty presents itself. We may be confident that cases of adaptive coloring exist but it is difficult to establish the adaptive nature of any particular case. Undoubtedly some cases of close color resemblance are accidental, in the sense of being unrelated to the external life of the organisms, so that it is only by multiplying instances that any general probability can be established. Again, while material of a general sort is available in large quantity, the number of clear cut types is much smaller. The exclusion of material rests upon various grounds, among which may be mentioned:

(*a*) The adaptation is to a very generalized background, and is usually accomplished by similarity of *pattern* as much as by hue. Thus the plumage of the quail and the markings of the garter snake serve admirably to conceal them in natural surroundings, but it is hard to pick out a pair of specific color matches. These cases, which are very numerous, can now be studied in the light of the knowledge of camouflage obtained during the war and some work of this kind has, I believe, been done. This work, which requires special training of the kind possessed by painters, seems capable of yielding very valuable information, but the results would be pertinent to the present inquiry only in so far as they might help to establish more generally the fact of the existence of color adaptations.

(*b*) The pair of colors is such as to offer little variety of possible composition. Reds and blues cannot be synthetized in as many ways as greens and yellows, so that the matches of these colors will necessarily show only minute spectral differences.

From this it follows that the most suitable material is furnished by color matches with a solid green or yellow background. The cases which were found most suitable were those of animals colored green to match the leaves and grass in which they lived. Such cases are very numerous among reptiles and insects and are particularly good instances because the green of chlorophyll has well marked spectral characteristics, being relatively deficient in the blue-green. On the other hand, many green insects assimilate directly in the epidermis the chlorophyll of the leaves on which they feed. Thus the katydid and the tomato worm are colored with actual chlorophyll. For such cases there would necessarily be no spectral difference and there is moreover not the same presumptive evidence of color adaptation. There remain, however, a large number of instances of which a few were studied.

The method of study adopted was by the use of color filters. These were used either (1) by illuminating the animal on its background with white light and viewing through the filter; or (2) by illuminating with the light trans-

mitted through filter. For the cases studied the two methods are equivalent, the first being suited to the study of organisms in their out of doors environment, and the second being on the whole more convenient in the laboratory. The two methods are not in all cases equivalent, as will be noted later.

The filters used were an improvised set made by fixing out and washing ordinary photographic plates and dipping them in dyes. These were made before going to Woods Hole and all dyes of the rather meagre stock available were tried out. These included some pre-war specimens of commercial "Diamond Dyes" and it is perhaps unfortunate that some of the most useful filters were made from these dyes of unknown composition. The most important filter is, however, Brilliant Green (Gruebler). Used in combination with Diamond Orange this transmits deep red and the blue end of the green. Seen through this combination foliage appears brownish red, whereas most artificial green pigments appear more intensely green. Used with Diamond Turkey Red the same general effect is produced, the obliterated portions of the spectrum being somewhat wider so that the greens appear bluer and the reds purer. Used with Diamond Olive Green, foliage appears straw color. Brilliant Green and eosin transmits only red and blue.

The most characteristic phenomena are shown by the common grass snake (*Liopeltis Vernalis*). The use of this animal, a specimen of which was secured early in the investigation, was suggested by the late Dr. George Lefevre, and saved much time which might otherwise have been spent in search for suitable material. This is to the eye so good a match for the grass which it inhabits that it is hard to discover the snake even when the observer is looking directly at it. When viewed through any of the filter combinations mentioned above the contrast is vivid and startling. The snake appears a vivid green, or, in the case of the eosin combination, deep blue—in absolute contrast to the red or yellow grass. A photograph, which can not here be reproduced, exhibits this contrast, the panchromatic plates used being more sensitive to the red than to the green, so that the snake appears darker; but no photograph can reproduce the violence of the color contrast. The green of frogs is of a similar character. Certain grasshoppers (*e.g. Melanoplus femur-rubrum*) have in their coloring a green that is spectrally different from leaf and grass greens, also the green of the so-called Florida chameleon (*Anolis Carolinensis*). This last changes from green to brown, but the coloring is not clearly adaptive.

These scattering observations and a few others were all that the time permitted last summer. While not sufficient to warrant definite conclusions they indicate clearly that color vision similar to human exists lower in the phylogenetic scale. It is hoped that next summer will furnish instances sufficiently numerous to settle this point definitely. In addition, many interesting lines of development open up, some of which will now be indicated.

Color is, as remarked above, not a physical but a psychological entity. To be green is to appear green to some percipient and it is the visual experience of this percipient that alone can explain the color in the sense in which we here use the word "explain". To make this sense clear it may be noted that though we may or may not have an explanation of the absorption spectrum of chlorophyll or hemoglobin, such an explanation would not touch the problem of the greenness of grass or the redness of blood. This being understood, the question arises: For what percipients does the color of a given organism exist? Now according as it can be shown to exist for the organism itself or for its environmental enemies or prey, we are led to biological implications of quite different kinds. In the first case we would be led to consider the evolutionary development of the adaptation as guided from without by natural selection or some similar process, whereas in the second case we are led to think of an evolutionary process self contained within the organism. These two points of view divide evolutionary speculation into two classes which we may call the peripheral and central types and the class of facts which these investigations open up seems suited to throw light on this very fundamental question. Keeping this philosophical problem in mind, let us enumerate some definite questions of fact which need to be settled.

(1) Just how low in the scale of organisms does color vision exist? The snakes and lizards have vertebrate enemies from which protection is needed. Most insects have birds to fear, so

that the few instances here studied give information only about the vertebrate eye. Cases of coloration among marine organisms are known which indicate color vision in anthropods, and the current explanation of the colors of flowers presupposes an ability of insects to discriminate color, but more information on this point is needed.

(2) Some animals have variable color and some at least of these make their own color response to visual stimuli. The most conspicuous example is the flat-fish. The very thorough studies of Mast and others establish the fact that this sees color, but no tests have been made to see whether, for instance, a red-green combination would be matched by its subjective equivalent, yellow. Here we have an animal which can actually be put through the same kind of psychological test that is applied to men. We ask it in effect, "What does this look like to you?" and tests can be made for after-image colors, contrast illusions, etc. Such tests seem likely to throw more light on color vision in animals than any other line that is now open. In this connection a point of technique in the use of color filters should be noted. It was said above that it is a matter of indifference whether the grass snake is illuminated with white light and viewed through the filter, or illuminated with the filtered light. This is true for an animal of fixed color, but it will be readily seen that the experience of the snake is different in the two cases, for in the second case it shares with the observer the color contrast produced. As a matter of fact the snake manifested no uneasiness at being thus rendered so startlingly conspicuous, indicating that color experience does not form a part of his ordinary response, though this can hardly be affirmed definitely on the basis of a single uncontrolled observation. In the case of an animal like the flat-fish, however, it is clear that the two methods of observation would lead to quite different results, for in the second case the fish would respond so as to make the contrast disappear as far as possible.

(3) Does color perception imply color *vision, i.e.,* are eyes essential to the process of color response? The question may seem absurd, but cases are cited in the literature of larvae forming chrysalides matching a background in which blinding the larvae did not alter the response. It may be that the underlying photochemical processes are here generalized in the skin instead of being confined to a specific end-organ. This would place color response on the same footing with most others, but the present meagre indications of fact must be widely corroborated and extended before any conclusions can be drawn.

(4) Do animals with dichromatic color vision exist? Is, for instance, the brilliant plumage of the cardinal really a contrast to the surrounding foliage for the eyes that are most concerned with finding him?

The writer hopes during the next and succeeding summers to continue work on some of the problems here raised.

This work was rendered possible by the generosity of the Nela Research Laboratories of the General Electric Company, who defrayed the expenses of the work at Woods Hole. To a physicist venturing into foreign fields the biologists there were most generous in their advice and encouragement. To Dr. Ralph S. Lillie in particular my thanks are due for his unfailing interest and helpful suggestions.

C. M. SPARROW

ROUSS PHYSICAL LABORATORY,
UNIVERSITY OF VIRGINIA,
FEBRUARY 28, 1923

LONGEVITY IN THE SOUTHERN STATES

THE federal statistics of 1920 are very important because they afford the first satisfactory opportunity of estimating longevity in the southern states. It is true that southern states were added to the registration area for deaths a few years before 1920, but as population statistics are collected only at decennial intervals the year 1920 is the first year at which both mortality and population statistics of the southern states are directly available.

As the negro has so often been referred to as the most disturbing factor in the progress of this country the results of an investigation into the relative longevity of southern states whose populations include such large numbers of negroes should be of considerable interest and importance. The writer believes that the general feeling among those interested in the problem has been similar to his own that such results would prove alarming. If so, the results

of this paper will prove quite surprising, for if the statistics of these southern states are reliable longevity in the south compares very favorably with longevity in the north. Those of us who have felt considerable pride in comparing longevity in this country with longevity in other countries have always felt a mental reservation on account of the possible conditions in the south since the latter were never represented in the comparison. It affords great relief then to find evidence that longevity in the southern states with their large proportions of negro population not only compares favorably with longevity in the northern states but even excels in some cases; that is, longevity in some of the southern states seems to excel quite significantly the longevity in some of the northern states. The writer hastens to add, however, that in his own estimation these conclusions are affected somewhat by consistent overstatements of advanced ages, at least, in most of the southern states. While it is impossible, of course, to measure the extent and effect of this tendency its existence is beyond question as is evidenced by the unnatural "bunching" of data in both the mortality statistics and the population statistics at the highest ages. However, the method followed in this investigation is one which minimizes inaccuracies at the most advanced ages, and considering that the first numerical measures of any natural phenomenon can be regarded only as first approximations the writer believes the numerical results given below to be very satisfactory for purposes of comparison with results obtained from other populations and drawing general conclusions therefrom.

The numerical results of this paper were ob-

MALES—EXPECTATION OF LIFE

Age	Ky.	Tenn.	Miss.	No. Car.	Va.	Florida	La.	So. Car.
10	54.2	52.5	51.0	52.1	51.8	50.1	50.0	49.5
20	46.1	44.6	44.5	43.9	43.7	42.3	42.1	41.6
30	38.5	37.4	37.7	36.7	36.3	35.2	34.9	34.6
40	30.7	30.2	30.4	29.5	29.0	28.2	27.9	27.8
50	23.0	22.9	22.9	22.2	21.8	21.5	20.9	20.7
60	15.9	16.0	15.8	15.3	15.0	15.2	14.8	14.2
70	10.1	10.3	9.9	9.4	9.4	9.8	9.6	8.9
80	5.8	6.0	5.8	5.4	5.5	5.6	5.6	5.3
Ave. Exp.	32.90	32.47	32.23	32.08	31.90	31.40	31.23	30.98

DEATH RATES PER 10,000

10	27	26	30	24	24	30	30	28
20	51	59	73	55	56	66	62	67
30	58	77	77	82	75	82	85	95
40	73	93	80	89	92	115	101	102
50	104	116	115	127	131	160	164	151
60	219	224	212	222	253	267	294	286
70	504	486	496	532	549	529	559	594
80	1086	1070	1202	1296	1312	1121	1153	1438

FEMALES—EXPECTATION OF LIFE

Age	Ky.	Tenn.	Florida	La.	Miss.	Va.	No. Car.	So. Car.
10	53.0	51.2	51.2	50.6	49.7	50.2	50.3	48.3
20	45.0	43.5	43.2	42.5	42.3	42.3	42.3	40.6
30	37.8	36.8	36.2	35.6	35.8	35.4	35.4	34.2
40	30.7	30.1	29.4	29.0	29.3	28.6	28.8	27.9
50	23.1	23.1	22.6	22.2	22.6	21.7	22.0	21.4
60	16.1	16.3	15.9	15.7	16.0	15.3	15.1	15.1
70	10.3	10.5	10.2	10.2	10.2	10.0	9.2	9.9
80	6.0	6.2	6.0	6.1	6.0	5.9	5.4	6.6
Ave. Exp.	32.68	32.20	31.97	31.72	31.68	31.50	31.48	30.98

DEATH RATES PER 1,000

10	26	28	25	25	33	27	23	29
20	58	69	64	63	78	66	65	80
30	83	97	89	97	103	94	98	112
40	80	100	107	108	110	107	116	127
50	111	132	138	152	149	147	127	160
60	219	210	226	256	219	270	234	270
70	475	475	502	500	489	517	521	564
80	1083	1063	1075	1098	1101	1128	1474	1237

tained by constructing abridged mortality tables from the mortality and the population statistics of 1920 of each of the eight southern states, Kentucky, Tennessee, Mississippi, North Carolina, Virginia, Florida, Louisiana and South Carolina for both the males and the females. Tables were constructed for both sexes also for the eight states taken as a whole and likewise for the ten northern states Connecticut, Indiana, Maine, Massachusetts, Michigan, New Hampshire, New Jersey, New York, Rhode Island and Vermont. Twenty abridged mortality tables then were constructed. The mortality tables themselves are not given, as practically nothing can be deduced from them which can not be deduced from the tables of expectations of life and death rates on the preceding page.

It will be noticed that the records of the various states vary in different ways at different ages; that is, one state may show up better at one interval of ages and another state at another interval. For that reason it is not easy to arrange the states according to their proper standing, as was attempted here. The scheme followed here was to compute what might be called the *average* expectation of life which is merely the average of the expectations of life at the given ages weighted by the number living at those ages as given by the abridged mortality table.

No detailed discussion of the expectations of life and the death rates at the different ages for the different states seems necessary. It is interesting to note, however, the excellent results of Kentucky and Tennessee, particularly Kentucky. Several years ago, the writer investigated the longevity of native Americans of native parentage and expressed then his belief that their record of longevity would compare favorably with the longevity of any other nationality. Measures of that longevity are closely approximated by the expectations of life for Kentucky. As the expectation of life at age 10 closely approximates that at age of birth in most mortality tables the expectation of 54.2 years for the males and 53.0 years for the females might be regarded as approximations of the average span of life of the two sexes in that state. It should be noticed that longevity in Kentucky seems to excel the average results for the ten northern states (see below) to a considerable extent. It is very remarkable that the records of the males seem to excel those of the females in five of the eight southern states (*i. e.*, in Kentucky, Tennessee, Mississippi, North Carolina and Virginia).

It is interesting to compare the expectations of life and the death rates of the ten northern states and of the eight southern states.

EXPECTATION OF LIFE

	MALES		FEMALES	
	North	South	North	South
10	52.3	51.7	52.9	50.7
20	44.0	43.8	44.6	42.9
30	36.1	36.5	36.9	36.1
40	28.4	29.3	29.3	29.4
50	21.0	22.1	21.8	22.4
60	14.3	15.3	14.9	15.7
70	9.0	9.8	9.5	10.1
80	5.2	5.7	5.5	6.0
Ave. Exp.	31.76	32.00	32.17	31.85

DEATH RATES PER 10,000

	MALES		FEMALES	
	North	South	North	South
10	30	27	26	27
20	45	60	46	67
30	62	77	69	96
40	83	92	81	106
50	132	130	122	132
60	281	246	249	237
70	604	526	568	502
80	1332	1191	1225	1124

It will be noticed that the expectation of life of the males of the northern states excel those of the males of the southern states only at ages 10 to 20 and that the average expectation of life of the southern males exceeds that of the northern males. It is a very interesting question to what extent the indicated superiority of the southern males at ages 30 and beyond is due to overstatement of ages. With the very worst interpretation comparison of the death rates of the two groups shows that the south compares very favorably with the north. The females of the two groups compare much the same way but the beginning of the indicated superiority of the southern females appears at a later decennial age than in the case of the males and hence the northern females show a slightly greater average expectation of life. It should be noticed that the southern males excel the southern females in the average expectation and in the expectation of life and

death rates at the earlier ages. Again it would be interesting to know whether and to what extent the indicated superiority of the southern females at the advanced ages is due to overstatement of advanced ages.

It should be stated that no scheme of smoothing of data was employed in computing the death rates given in the tables above and that the computation of each death rate was essentially independent of that of any other. In the light of this fact the absence of significant incongruities and the general agreement of the various numerical results at the different ages of all twenty mortality tables is rather remarkable. It is only hoped that the evidences of overstatement of advanced ages—and the northern states are not wholly beyond criticism in this respect—will gradually disappear. It should also be added that since all of these mortality tables were constructed from the statistics of a single year (1920) the results given above are offered merely for purposes of comparison and *general* conclusions.

C. H. FORSYTH

DARTMOUTH COLLEGE

SCIENTIFIC EVENTS

SAFETY CODE FOR AUTOMOBILE BRAKES

A NATIONAL safety code for automobile brakes and brake testing will be developed under the auspices of the American Engineering Standards Committee as the result of the decision of a special committee consisting of representatives of the American Automobile Association, the Motor Vehicle Conference Committee, the Conference of Motor Vehicle Administrators, the American Society of Mechanical Engineers, the United States Bureau of Standards, the United States War Department, the Society of Automotive Engineers, the Underwriters Laboratories, the Safety Group of the American Engineering Standards Committee and David Beecroft of the Class Journal Company. David Van Schaack, vice-president of the National Safety Council, was chairman of the committee.

The American Engineering Standards Committee has asked the Society of Automotive Engineers, the Bureau of Standards and the American Automobile Association to act as joint sponsors for the development of this code. The actual work of formulating the code will be carried on by a sectional committee representative of all the interests concerned with the problems of automobile brakes either as administrative officers, users, producers or general public.

The need for a national safety code covering methods of testing brakes and also those elements of brake or brake lining construction which are accident factors is emphasized by the increasing tendency toward state and municipal legislation on this subject. Several states already have brake testing laws and in a number of other states such laws are being drafted. There is, however, no agreement at present as to what is a safe braking distance or what constitutes a satisfactory braking test. Until a national safety code has been developed and generally accepted the motorist driving from one city to another or from state to state will have no assurance that brakes approved in one city will not bring a fine in the next town or state.

THE AUSTRALIAN NATIONAL RESEARCH COUNCIL

THE Australian National Research Council has issued a report of its annual meeting held in Sydney in August last which is summarized in *Nature*. The council was formed for national and international purposes in January 1921 by the Australian Association for the Advancement of Science, to which body it has to submit a full report of its work and proceedings on the occasion of each meeting of the Association. At the meeting Sir David Orme Masson was elected president of the council in succession to Sir Edgeworth David. Resolutions were passed urging the need for the State endowment of systematic research in the Pacific islands under Australian control, for research work in Australia in respect of refrigeration, and for laboratories to carry out industrial investigation and research. Offers of co-operation with the Commonwealth Institute of Science and Industry in measures for furthering these objects were made, and preliminary steps taken for the inauguration of a publicity campaign for the purpose of securing that the functions, operations and financial needs of the

Institute may be more fully appreciated by the Commonwealth Government, the Legislature and the public generally. The council has decided to ask the Australian Association to regard it as a fully constituted body free to conduct its own affairs subject to instructions from the International Research Council. The first issue of *Australian Science Abstracts,* published by the Australian Research Council as a quarterly journal of abstracts of papers by Australian scientific workers, appeared on August 1, 1922. An invitation has been issued by the Commonwealth Government through the Research Council to the representatives of the Pan-Pacific Scientific Congress to hold the Congress in Australia in 1923.

ENGINEERING LIBRARY FOR DENVER

Scientific circles will recognize the importance of the library facilities that have just been opened for engineers and scientific men in the City of Denver.

The Colorado Scientific Society has had a long history and has been the medium through which many discoveries, especially along the line of geology and mineralogy, have been announced. In the past few years this organization has not been as active and its financial means have not enabled it to carry on work to the same degree as formerly.

It has been given quarters for a number of years by the State of Colorado in the State Museum Building, and many of its museum specimens have been cared for by the State Bureau of Mines. Through exchanges and through purchase and through gifts from members, it had acquired a library especially valuable in transactions of learned societies, in scientific exchanges and in engineering books.

The engineers and men of applied science felt the increased need of a library for reference. There is no important library of this kind available for practically a thousand miles. The Rocky Mountain region has a large number of different lines of science represented by men of ability, but has lacked these facilities.

The various engineering societies, especially the twelve which are grouped together and form the Colorado Engineering Council, have been active in pushing the idea of the union of forces and starting a library of this character.

The State of Colorado passed a few years since a law requiring all practicing engineers to register and pay a license fee. The funds from this source have amounted to some $15,000. The cooperation of this board was obtained and these funds made available for the purchase of books, which could be done under State laws, and the books used in connection with the books of the Scientific Society and other books, the title remaining in the State.

The Society of Architects which had a fund of some thousands of dollars, has made their fund available for this technical library. Many books have been given by individual engineers who find it unnecessary to keep in their private collections so many works if they are available to them at any time in a library.

The Denver City Library has assigned room and a technical librarian and takes supervision and general charge of the collection.

The dedication exercises were held on April 7, with representatives from the Scientific Society, engineering professions, city of Denver and the State of Colorado.

This library has some thousands of volumes to start with, and an assured income of several thousand dollars. The prospect is that this will be the most important center for a technical library and applied science between Chicago and the Pacific Coast.

L. G. CARPENTER, *President.*
COLORADO ENGINEERING COUNCIL.

NEW YORK BOTANICAL GARDEN LECTURES

During the spring and summer lectures are being given on Saturdays and Sundays at 4 p. m. in the lecture hall of the Museum Building as follows:

April 28. "Spring wild flowers," Dr. G. C. Fisher.

April 29. "Pasteur and the founding of bacteriology," Dr. G. M. Reed.

May 5. "Books on gardening," Dr. J. H. Barnhart.

May 6. "Bees and bee culture," Professor G. C. Norton.

May 12. Nature talks—II. "Rocks," Dr. W. A. Murrill. (Exhibition of spring flowers, May 12 and 13).

May 13. "Daffodils and tulips," Mr. K. R. Boynton.

May 19. "Practical hints on garden design," Miss E. L. Lee.

May 20. Nature talks—III. "Soil," Dr. W. A. Murrill.

May 26. "The tea gardens of Ceylon and Japan," Dr. H. A. Gleason.

May 27. "Wild flowers," Mr. L. W. Brownell.

June 2. "Economic uses and possibilities of seaweeds," Dr. M. A. Howe.

June 3. "Evolution," Dr. C. S. Gager.

June 9. "Insects and insect fungi," Dr. F. J. Seaver.

June 10. Nature talks—IV. "Ferns," Dr. W. A. Murrill.

June 16. "Summer wild flowers," Mr. C. W. Johnson.

June 17. "Roses," Mr. Robert Pyle.

June 23. "The big trees of California," Dr. H. A. Gleason.

June 24. "The history and sources of quinine," Dr. H. H. Rusby.

June 30. "Flowers of the four seasons," Mr. K. R. Boynton.

July 1. Nature talks—V. "Mushrooms," Dr. W. A. Murrill.

July 7. "Plant traps," Dr. J. H. Barnhart.

July 8. "Some great men and their love of trees," Mr. Carl Bannwart.

July 14. "Diatoms—plants of beauty seen through a microscope," Dr. M. A. Howe.

July 15. "A garden pilgrimage in England," Mr. Montague Free.

July 21. "Collecting fungi in Porto Rico," Dr. F. J. Seaver.

July 22. Nature talks—VI. "Punk-fungi," Dr. W. A. Murrill.

July 28. "Fifty trees in the botanical garden," Mr. K. R. Boynton.

July 29. "Evergreens: what to plant and where to plant them," Mr. Arthur Herrington.

FORESTRY RESEARCH

AN anonymous donor has given $100,000 to Harvard University and an equal sum to Yale University to establish endowment funds for research work on forestry problems.

The income of the fund offered to Harvard, it is stipulated, is to be used for research and experimental work in forest production, "to determine the basic facts essential to the successful practice of silviculture in the northeastern section of the United States, especially in New England." The Harvard forest, consisting of three tracts of land in Petersham totaling over 2,000 acres, has been maintained for fourteen years under the direction of Professor Richard T. Fisher, to serve as a model forest to demonstrate the practice of forestry, as a field laboratory for students, and also as an experiment station for research. The forest has in operation practicable methods of reproducing and improving forests; it has large plantations under test for varying situations; and it is investigating forestry problems of all sorts. The expectation at Harvard is that the new gift will make it possible to develop this scientific work to a much greater extent than has hitherto been possible, and will thus aid in securing the technical knowledge needed to handle forest crops effectively in the face of an impending national timber shortage.

For several years Yale has owned forests in Connecticut, New Hampshire and Vermont upon which some experiments are already being carried out. The Northeast section of the United States affords the best opportunities for demonstration of the practice of forestry. The best markets are here, and the greatest need of intensive field work.

According to Henry S. Graves, dean of the Yale School of Forestry, and formerly chief of the United States Forest Service, the new gift will make possible a large increase in the field studies in the growth and production of pine on the Keene experimental forest in New Hampshire and of hemlock and hardwoods on the Connecticut properties. Professor James W. Toumey and Professor Ralph C. Hawley will supervise the research work.

SCIENTIFIC NOTES AND NEWS

SIR DAVID BRUCE has been nominated by the council as president of the British Association for the Advancement of Science at its meeting next year in Toronto.

THE National Academy of Sciences has presented its Comstock Prize to Professor William Duane, of Harvard University, in recognition of his researches on X-rays, and its Mary Clark Thompson Gold Medal to Dr. Emmanuel de Margerie, director of the Geological Survey of Alsace and Lorraine.

AT the April stated meeting of the Franklin Institute, held on the eighteenth instant, the Howard N. Potts Gold Medal was presented to Dr. Albert W. Hull of the Research Laboratory, General Electric Company, Schenectady, New

York, for his paper on "The crystal structure of the common elements." At the same meeting the Edward Longstreth medal was presented to the Société Genevoise d'Instruments de Physique of Geneva, Switzerland, for their universal measuring machine. This medal was received by Mr. Roy Y. Ferner, of Washington, D. C., the American representative of the society.

THE Matteucci gold medal, conferred as a posthumous honor by the International Research Council at Brussels in 1919 on the late Mr. H. G. Moseley, of Trinity College, Oxford, has been delivered to his mother.

WORD has been received from Holland that Dr. L. O. Howard, of Washington, has been made honorary president of the International Conference of Phytopathologists and Economic Entomologists which is to be held June 24th at Wageningen, Holland. While in Europe, Dr. Howard will attend the International Congress of Agriculture at Paris May 22-26, and the international conference concerning *Dacus oleae* to be held at Madrid on June 18.

PROFESSOR CHARLES PALACHE has been appointed curator of the mineralogical museum at Harvard to take office next September, in place of Professor John Eliot Wolff, whose plan to retire from active teaching next summer was recently announced.

DR. CHARLES SCHUCHERT, professor of paleontology and historical geology at Yale University since 1904, and Dr. Horace L. Wells, professor of analytical chemistry and metallurgy since 1893, will retire from active teaching in June.

ACCORDING to an announcement recently issued by the board of conservation and natural resources of the state of Illinois, Dr. Morris M. Leighton has been appointed chief of the State Geological Survey, to succeed Frank W. DeWolf, who resigned on March 20. Dr. Leighton, who is at present associate professor of geology at the University of Illinois, has for several years maintained a part time connection with the Illinois State Survey, carrying on during the summers investigations, both scientific and economic, relating to the surficial deposits of the state.

DR. EDWARD P. HYDE, who organized the Nela Research Laboratories in 1908 and who in recent years has occupied the position of director of research of the National Lamp Works of the General Electric Company, has tendered his resignation to take effect June 30 of this year. Dr. Hyde will take a prolonged rest abroad.

DR. HENRY GORDON GALE, professor of physics and dean of the Ogden Graduate School of Science of the University of Chicago, has been elected a director of the John Crerar Library.

HARRY N. WHITFORD, assistant professor of tropical forestry at Yale University, has been granted leave of absence to assist the United States Department of Commerce in the investigations in the production of rubber recently authorized by Congress.

APPOINTMENT of W. L. Abbott, of Chicago, as chairman of a committee to make a nation-wide coal storage survey in cooperation with the United States Coal Commission and the Department of Commerce is announced by the Federated American Engineering Societies.

DR. FRANK APP has resigned from the Agricultural College and Experiment Station of Rutgers College to become vice-president of the Minch Brothers Company at Bridgeton, N. J.

COLONEL CHARLES KELLER, of the Corps of Engineers, U. S. A., who has served for the past two years as engineer commissioner of the District of Columbia, has resigned to undertake work in connection with hydro-electric development on the Pacific Coast.

PROFESSOR AND MRS. T. D. A. COCKERELL, of the University of Colorado, will sail from San Francisco for Yokohama on June 4. Thence they will proceed to Vladivostok, in order to investigate a deposit of fossil insects recently discovered on the coast of Siberia, returning to America in September.

SIR WALTER BALDWIN SPENCER is about to lead, on behalf of the Australian government, a mission into Central Australia to investigate the best means of improving the conditions of the aborigines.

CAPTAIN ROALD AMUNDSEN left Nome, Alaska, on April 13, by dog team for Wainwright, five hundred miles north, in preparation for an attempt to fly over the North Pole to Norway.

S. R. WILLIAMS, professor of physics of Oberlin College, is spending a year's leave of

absence at the California Institute of Technology. At a recent public meeting held in the auditorium of the Norman Bridge Laboratory of Physics, Professor Williams spoke on the subject of "Magnetism" and gave a public demonstration of the Barkhausen effect by means of a power amplifier and a "Magnavox."

DR. ALFRED MARSTON TOZZER, of Harvard University, is in residence at Pomona College until May 10, giving a series of lectures on the Johnson foundation. He will deliver a series of three lectures on anthropology, three lectures on the "Evolution of man" and three popular evening lectures.

DR. T. C. MENDENHALL, of Ravenna, Ohio, delivered the annual address before the Ohio Eta Chapter of Phi Beta Kappa, Ohio Wesleyan University, on April 9. His subject was "Weighing the earth."

PROFESSOR ALBERT A. MICHELSON, of the University of Chicago, will lecture before the Brooklyn Institute of Arts and Sciences, Friday evening, April 27, on "The measurement of stars by light waves."

AT a meeting of the Washington alumni chapter of Phi Kappa Phi on April 11 an address was delivered by Dr. E. W. Allen on "Pasteur: a lesson in productive scholarship".

DR. PAUL S. WELCH, of the University of Michigan, national president of the Gamma Alpha Graduate Fraternity, will lecture on April 27 under the auspices of the Ohio chapter on the subject, "Limnological investigations on Michigan lakes."

PROFESSOR LOUIS KAHLENBERG, of the department of chemistry of the University of Wisconsin, was at the University of Missouri at Columbia on April 4 and 5 and at the School of Mines at Rolla on April 5 and 6. He lectured at both places on his experimental researches on the separation of crystalloids by dialysis. He also spoke at convocation on recent tendencies in scientific research.

DR. KAREL FREDERIK WENCKEBACH, professor of medicine, University of Vienna, will deliver the eleventh Harvey Society lecture at the New York Academy of Medicine on Saturday evening, April 28. His subject will be "The rhythms of the heart."

DR. KENNETH C. HEALD, of the United States Geological Survey, geologist in charge of the section of geology of oil and gas fields, is on leave of absence from the Geological Survey, delivering a course of lectures on petroleum geology at the University of Chicago.

AT a joint meeting of the American Association for the Advancement of Science, Sigma Xi and Phi Kappa Phi at the Pennsylvania State College on Friday, April 20, Herman Stabler of the United States Geological Survey spoke on "The canyons of the Colorado River" and Professor J. E. De Camp discussed "The mental tests at State College" which have been given by the department of education and psychology.

IN accordance with the plan adopted last year of throwing open to the public a series of lectures on various topics in the field of physics, Harvard University, through its department of physics, has arranged to offer this spring three popular lectures, as follows: April 27, "Sound waves," with experimental illustrations, Professor F. A. Saunders; May 4, "Atomic explosions," Dr. Kemble; May 11, "Electron tubes, amplifiers, detectors and oscillators," Professor E. L. Chaffee. Each of these lectures will be held in the large lecture room of the Jefferson Physical Laboratory and will begin at 8 p. m.

THE Friday evening meetings of the Royal Institution, London, were resumed on April 13, when the discourse was delivered by Dr. W. H. Eccles on "Studies from a wireless laboratory." Succeeding discourses will probably be given by Mr. W. J. S. Lockyer, Mr. C. V. Boys, Professor Soddy, Professor W. A. Bone, Mr. W. M. Mordey, Sir Aston Webb, Professor Lorentz and Miss Joan Evans.

LEGISLATION against teaching "as true" either "Darwinism or any hypothesis that there is blood relationship between man and the animals," was urged by William Jennings Bryan in an address to the West Virginia legislature on April 13.

TWO fellowships, of 12,000 liras each, are offered by the Italian government for research on malaria. Among the themes suggested for special study are the development of malaria in regions with few mosquitoes; the biology of mosquitoes; their range of flight; the incidence

of malaria before the mosquito season; relations between malaria and domestic animals; the water plants which promote or check proliferation of mosquitoes; other larvacidal measures and means to enhance immunity and protect workers in the fields.

THE University of Chicago board of trustees announces that the Metropolitan Life Insurance Company has again renewed its grant of $3,000 to the department of hygiene and bacteriology for special investigations in respiratory diseases.

UNIVERSITY AND EDUCATIONAL NOTES

THE University of Minnesota has received a gift of $250,000 with which to erect and equip a cancer hospital to be a part of the University of Minnesota General Hospital. The money was donated by the Citizens Aid Society of Minneapolis, established in 1916 by the late George H. Christian.

EIGHT members of the faculty of the Women's Medical College of Philadelphia presented their resignations to the board of incorporators of the institution on April 17 in protest against the dismissal of Dr. Alice W. Tallant, head of the obstetrics department. This made a total of fifteen resignations, including the entire surgical staff, for the same cause. Incompatibility was assigned as the reason for Dr. Tallant's dismissal.

THE resignation of Dr. John P. Sutherland as dean of the Boston University School of Medicine, after twenty-six years of service as the head of the school, has been accepted by the trustees, and Dr. Alexander S. Begg, formerly dean of the Harvard Graduate School of Medicine, now professor of anatomy at the school, has been appointed dean for the coming year.

AT Yale University Adolph Knopf, associate professor of physical geology and petrology, has been promoted to be professor; George E. Nichols, assistant professor of botany, to be associate professor; and Charlton D. Cooksey and Alan T. Waterman, instructors in physics, to be assistant professors.

DR. J. H. VAN VLECK, now at Harvard, and Dr. Gregory Breit, who this year is National Research Council fellow in physics, also at Harvard, have been appointed to assistant professorships at the University of Minnesota department of physics following the resignation of Professor W. F. G. Swann to go to the University of Chicago. Dr. Van Vleck, a son of Professor Van Vleck, of the University of Wisconsin, has worked on atomic structure. Last year Dr. Breit conducted research in physics at the University of Leyden.

MR. R. S. ADAMSON, lecturer in botany at Manchester, has been appointed to the Harry Bolus chair of botany in the University of Cape Town.

DISCUSSION AND CORRESPONDENCE

MARINE ZOOLOGICAL STATIONS

I AM sorry that my recent letter to SCIENCE[1] regarding work in marine zoology seems to have caused Dr. Fisher[2] uneasiness. Perhaps I can try here to indicate more clearly what I had in mind.

A number of the points raised in Dr. Fisher's comment were familiar to me, and do not cause me to change my original view. Perhaps the most important matter involves the proposition that one can not continuously pursue intellectual work in a warm climate, and that the "glamor" fades. My own belief, based upon a certain amount of direct observation, is this: that a person, biologist, who will loaf in (say) Bermuda, will probably loaf in any other place. The notion that all biologists not in the subtropics work continuously and at high intellectual pressure would certainly be grotesque! As Peterson remarks, in speaking of his bottom sampling operations, to carry on research it is necessary not only to have equipment and the opportunity for using it; it is necessary also to have ideas. So that if there were an inevitable incompatibility between warm weather and cerebration, for the majority of persons, it would surely be unfortunate. But aside from Beebe's pronounced expressions of opinion on this subject, I may point to the fact that Dr. Fisher's view, as he has stated it, does a quite unmerited injustice to workers at Manila, in Java, in

[1] SCIENCE, December 29, 1922, p. 751.
[2] *Ibid*, February 23, 1923, p. 233.

India, in some parts of South Africa, and in Hawaii, and in South Australia.

Apart from all this, the thing which I chiefly strove to emphasize is the fact that a marine laboratory can hardly hope to justify itself as an investment if it relies solely upon the sporadic visits of biologists recuperating from a siege of instructional duty. It seems to me that there is most urgent need for a station, suitably located, which may hope to do the sort of thing which Naples did and which Plymouth is now doing, and in an environment of the most advantageous character. Convenient accessibility is highly desirable. One can hardly speak of Pacific Coast laboratories as "accessible" for the majority of American biologists; it is cheaper, or as cheap, to go to the nearer parts of Europe.

But in my letter I spoke of "accessibility" in another sense, with reference to a varied fauna. I have in mind especially the conditions at Bermuda, but I doubt not that other locations equally favorable can be found. Within less than an hour's run in a small boat from the Bermuda laboratory and in most cases closer than this, one could reach exposed or sheltered shores of sandy, muddy, or rocky type, caves, mangrove creeks, lagoons, and the locations of four or more distinct types of coral associations, each with its characteristic fauna, and all of them free from pollution; and this at all periods of the year. I doubt very much if those who have not tasted the experience of continuous biological work (not merely collecting of specimens) under such conditions have a proper realization of its possibilities. The abundance of this fauna can be appreciated only by living with it. It seems to me stupid that the opportunity to further the advancement of biology in locations of this sort has not been seized.

W. J. CROZIER

ZOOLOGICAL LABORATORY,
RUTGERS COLLEGE

A PROGRAM OF OCEANIC INVESTIGATIONS

TO THE EDITOR OF SCIENCE: Like Dr. Fisher (SCIENCE, February 23, 1923, p. 233) I was much astonished at some of the assertions made by Dr. Crozier in SCIENCE for December 29, 1922. I can not hope to add to the admirable lucidity of Dr. Fisher's comment, but it does seem desirable to add special protest against Dr. Crozier's scornful dismissal of the idea of a "program" of oceanic investigation.

I have recently read Raphael Pumpelly's "Reminiscences" and I am now reading Jordan's "The Days of a Man." From these and other sources I get the overwhelming impression that when great problems of science have been successfully attacked by these men and their fellows there was in mind a definite program (or plan) of operations. As I understand it, the failure of the brilliant Rafinesque was primarily due to lack of plan.

The general difficulties of oceanic investigation seem to me to be somewhat similar to those of astronomical investigation. I can not imagine any one seriously suggesting that the Harvard, or the Yerkes, or the Lick, or the Mount Wilson Observatory should work without a program. Furthermore, what less than a program of some sort could justify the installation of a "stationary staff," or, indeed, make it possible to intelligently select such a staff?

Those of us who work day by day and year after year in the La Jolla laboratory are frequently and vividly impressed by the constant intercrossing of lines of influence between our respective fields and between indefinite numbers of minor points in each field. A major stimulus to continued investigation is the hope that we shall some time get enough accurate information to enable us to trace many of these lines of influence through the intricate web. This hope would be denied to us if we ignored the idea of a general program for all of our oceanic research.

Indeed, I know of nothing more satisfying to the scientific staff of the Scripps Institution than the fact that a program of some sort has been followed from the beginning and that a *permanent* program now seems to be definitely recognizable. All along one of the most prominent elements in our program has been the study of natural (in nature) activities of marine organisms as distinguished from (or at least compared with) circumscribed activities of such organisms in laboratories. Several years ago both Michael and Esterly showed through a series of statistical examinations of the marine population that laboratory deduc-

tions are not safe guides to interpretation of the intricacies of life in nature. McEwen's work in hydrography has even more strikingly shown the inadequacy of laboratory deductions. The success of all such studies has been largely due to following a program of operation.

Recently, Dr. O. T. Wilson, of the University of Cincinnati, arrived here with the statement that he was not committed to any special study and that he would like to do something to fit in with our work. As a result he is making some studies of floral successions on various kinds of surfaces in shallow waters with excellent results and with pleasure surely not to be surpassed by ignoring a general idea of plan in oceanic work.

W. E. ALLEN

SCRIPPS INSTITUTION,
LA JOLLA, CALIFORNIA

DEFINITIONS IN TEXT-BOOKS OF PHYSICS

I WISH to enter a protest, through your columns, against the continued use of two common expressions in text-books of physics, both of them true if properly interpreted, each of them capable of various interpretations and therefore inexact, unscientific and pedagogically wrong.

Year after year students tell us on the authority of text-books that "efficiency equals output over input," and year after year some of those students apply the idea to forces and confuse efficiency with mechanical advantage. "Output/input" is a form which lends itself to use in concise summaries of chapters on mechanics, but it is a stumbling-block in the path of just those careless or undiscriminating students who will most readily grasp that form of words in lieu of an idea. Such an expression as "output/input" means nothing in itself, because it might refer to anything, whether work, force, mass, volume or time; it means nothing unless it is defined, and if you must define it, why not use the definition directly? To be sure, it affords joy for a moment to the heart of a certain type of student, who, after struggling through a discussion of work as applied to machines till his brain is surrounded by a semi-luminous fog, turns over a page and suddenly sees a magic formula which he follows as a bright and shining light, a will-o'-the-wisp which lures him on to worse confusion than before.

The other practice to which I object is that of putting Newton's third law of motion into the form "Action = Reaction." What does it mean standing by itself on the page of an elementary text-book? It might refer to force, or to work, or to momentum, or even to that "action" whose leastness troubles the students of more advanced texts. Even the authors of the first book in which I ever saw a clear exposition of the third law of motion took pains to place before their own clear statement of the law the statement that Newton had put it in the form "Action is equal to reaction." Apparently even they, eminent physicists and understanding teachers as they are, felt obliged to pour out a libation to the shades of the original translators of Newton in the traditional way. My own experience in trying to teach with that book as a text has been that invariably some of the men who stood in greatest need of the clear statement were sure to learn the shorter and catchier statement which meant nothing to them. In some books the old form is the only formal statement of the law given, and, in spite of good illustrative explanations when they are given, it serves fairly effectually to prevent an understanding of the law as distinguished from an ability to parrot off a formula.

I am aware of the usefulness of very concise or semi-algebraic forms for gathering up and emphasizing the important points in a chapter, but there is also a grave danger of increasing the tendency of some students to fail to discriminate between a form of words as such and the real physical idea associated with it. This tendency is present even in the case of formulas and other very concise forms of statement which are perfectly accurate and definite, and is greatly aggravated in the presence of forms whose intended meaning does not necessarily follow from the words used. Two of the worst of the latter are those two which I have mentioned, and I should like to see them relegated by common consent to outer darkness, not afforded even the meager hospitality of a foot-note.

A. B. MESERVEY

DARTMOUTH COLLEGE

QUOTATIONS

MEDICAL RESEARCH

MEDICAL research is of so continuous a nature that attempts to review its progress at any given time are necessarily fraught with great difficulty. Observations and experiments which to-day assume large proportions may to-morrow seem relatively insignificant in the light of clearer understanding. On the other hand, labors which at this present hour appear to hold out but little promise of immediate utility may come to occupy a first place in the records of achievement. These facts should be borne in mind in considering the annual report of the Medical Research Council, a summary of which appears in another column. The number of investigators initiated or assisted by the council during the past year is so great as to be almost bewildering. These investigations, too, cover a field within the limits of which are included many other sciences distinct from medicine, yet, as is now known, ancillary to it. If certain observations appear to stand out from the others, and in consequence attract a larger share of attention, this fact must not be allowed to discount the possibility that the more obscure may yet prove to be the more important work. That proviso having been made, it may be acknowledged that the researches of the past year have been fruitful. New light has been obtained on some of the more recondite processes of nutrition. The importance of sunlight in the early years of life has found substantial and striking confirmation in the study of rickets carried on at the Kinderklinik in Vienna by workers from the Lister Institute and the Medical Research Council. Further, the discovery made at Toronto of a method of obtaining the anti-diabetic substance "insulin" seems likely to effect great changes in the treatment of this disease. That discovery belongs in no sense to the Medical Research Council, which merely recounts its history and pays tribute to its value. Nor, as our correspondent shows, have other scientific bodies been idle during the period under review. The papers submitted last night to the Royal Society of Tropical Medicine and Hygiene on the treatment of African sleeping sickness, if they do not profess finality, at least advance this difficult problem a stage farther towards solution. The German drug known as "Bayer 205," which was introduced to professional attention some short time ago, has afforded in the hands of British tropical disease investigators a large measure of success. Its failures are not less interesting or instructive; nor is the parallel between it and the preparation of arsenic, elaborated at the Rockefeller Institute and recently subjected to trial in Africa, without its significance. These observations, like the observations made at home and in temperate areas of the empire, have abiding value, whether or not the conclusions based on them remain valid. For they represent conscientious and disinterested work undertaken with no other idea than the advancement of knowledge. This quality of disinterestedness is the noblest possession of science. So long as it is maintained unsullied the faith which millions to-day repose in the exercise of human reason as the cure of human ills will not fail of justification.—*The London Times.*

SPECIAL ARTICLES

THE MINIMUM CONCENTRATION OF LUCIFERIN TO GIVE A VISIBLE LUMINESCENCE

LUCIFERIN is the substance of luminous animals which produces light when oxidized. An enzyme, luciferase, likewise found in luminous animals, must be present with luciferin for light production. In 1916[1] I calculated that one part of luciferin from an ostracod crustacean, *Cypridina,* in 1,700,000,000 parts of sea water, when mixed with luciferase, gave a light visible to the unaided eye. This figure is so much smaller than the concentration of substances detectible by ordinary chemical reactions, which at most run into the millions, that I have repeated these experiments, only to find that my preceding estimate may be bettered. The light from weaker solutions of luciferin than 1:1,700,000,000 can be seen.

In this work an important source of error has appeared which must be carefully guarded against and to which I call special attention below. The exact procedure of determining the minimum concentration is as follows: 0.0954 grams of dried *Cypridinae* are weighed out, ground in a mortar, dissolved in boiling[2] sea water and quickly diluted to 954cc cold sea

[1] *Amer. Jour. Physiol.,* XLII, 335, 1917.

[2] Boiling destroys the luciferase, but not the luciferin.

water.[3] When filtered this forms the stock solution which contains one part dry *Cypridina* in 10,000 parts sea water. One part stock solution to 99 parts sea water gives solution A, that is, 1:1,000,000 solution, and one part of solution A to 99 parts sea water gives solution B, that is, 1:100,000,000 solution. If A is mixed with an equal volume of luciferase solution,[4] quite a bright light results. If B is mixed with an equal volume of luciferase solution,[4] a faint light results. B/2 gives a very faint light and B/4 gives a light so faint that the fully dark adapted eye can hardly detect it. Some sets of observations were made by myself and some by Mr. K. P. Stevens. We remained in a dark room for at least three quarters of an hour in order to increase the sensitivity of our eyes. In mixing the solutions, 5 c.c. of luciferase was poured into 5 c.c. luciferin in a test tube, 18 mm. in diameter. If we assume that B/2 is the solution giving light just detectable with the eye, note that its concentration is halved on mixing with an equal volume of luciferase, so that we can without doubt see the light from a B/4 or a 1:400,000,000 solution of the substances in dry *Cypridina*.

The remainder of the calculation depends on the per cent. of luciferin in dry *Cypridina*. It must surely be less than 10 per cent. and, judging by the size of the luminous gland, greater than one per cent. We can, therefore, safely say that one part of pure luciferin in between 4 and 40 billion parts of sea water gives, upon oxidation, light visible to the unaided eye.

A similar experiment with luciferase shows that one part of pure luciferase in between 800 million and 8 billion parts of sea water will oxidize a stock luciferin solution with visible luminescence.

In the above work new glassware that had never been used for luciferin, or in fact for any other purpose, was employed. This is necessary as can be seen from the following experiment. A 100 c.c. graduate cylinder which had held the stock solution of luciferin (1 part dry *Cypridina* to 10,000 sea water) was washed in fifteen changes of hot water (not too hot for the hands). The graduate cylinder was then filled with sea water which stood in the cylinder for two minutes and was then tested with luciferase. A very faint light appeared. After standing five minutes the light from the sea water in the cylinder mixed with luciferase was brighter and after standing ten minutes quite a fair light was produced. Sea water which had not stood in this graduate cylinder gave no light with luciferase.

Evidently luciferin adsorbed on the walls of the cylinder is slowly liberated into the sea water. It is not removed by mere washing in fifteen changes of hot tap water. If I had used this particular graduate, supposedly clean, to hold any of my dilute solutions of luciferin it is obvious that serious error would have appeared in determining the minimum concentration of luciferin for luminescence.

Adsorption is no new phenomenon. We allow for it in dealing with the large surface areas of powders but generally overlook it on the surface of measuring vessels. However, adsorption is not to be neglected in an experiment dealing with parts per billion. The adsorbed body is especially likely to come off if the vessel is used with another solvent. Dye on the surface of glass that will not come off on washing in water may be removed by washing in alcohol or in slightly alkaline or slightly acid water.

I would suggest that the extraordinarily small concentration of Botulinus toxin necessary to kill mice (3×10^{-18} c.c. regularly and 3×10^{-21} c.c. sometimes) as reported by Bronfenbrenner and Schlesinger[5] may possibly be explained by "seepage" of adsorbed toxin from vessel walls in some such way as I have just indicated. One can not be more specific in pointing out any error in this work as the method of dilution of the crude toxin is not described in detail. A concentration of 10^{-21} c.c., however, requires careful scrutiny.

It is interesting to compare the minimum concentration for luciferin luminescence with the concentration of colored solutions at which the color is just perceptible. Mr. Stevens has made some determinations with dyes, such as methy-

[3] Rapidity in making dilution is essential to reduce spontaneous oxidation of luciferin to a minimum.

[4] 0.1 gram dry *Cypridina* to 100 c.c. cold sea water allowed to stand until luminescence ceases and filtered.

[5] *Jour. Am. Med. Assoc.*, 78, 1519, 1922; SCIENCE, LVI, 280, 1922; cf. Stehle, SCIENCE, LVI, 143, 1922.

lene blue, acid fuchsine, gentian violet, methylene green, etc., in 18 mm. test tubes observed in the daylight from the side, and finds the limit of color visibility in the ten millions, one thousand times above the limit for luciferin luminescence. There is one exception, which concerns not transmitted light, but fluorescence. A faint greenish fluorescence of fluoresceine is clearly visible in the light from a powerful carbon arc focussed in the solution, with one part of the dye in 10 billion parts of ordinary distilled water. Such water is not by any means optically empty and light scattering from dust particles interferes with the observation of fluorescence. Spring[6] has reported that with optically empty water, the fluorescence from 10^{-15} grams per c.c. of fluoresceine can be detected in the beam of an arc lamp.

Thus, luminescences from very small quantities of matter can be detected. We have an extreme case in the flash of light from impact of a single charged helium atom on fluorescent zinc sulphide. Biological reactions can not approach this in sensitivity, but I believe the value for *Cypridina* luciferin mentioned above sets a new record for chemiluminescence.

E. NEWTON HARVEY

PRINCETON UNIVERSITY,
MARCH 31, 1923

X-RAYS AND THE SEX CHROMOSOMES

IN two previous issues of this journal[1] the writer has reported what appeared to be the production of non-disjunction of the sex chromosomes of Drosophila by X-rays. When primary non-disjunction is found, as happens rarely, in ordinary cultures the two kinds of exceptional offspring do not occur in equal numbers. The exceptional sons which arise from the fermentation of eggs containing no X-chromosome by sperm containing an X-chromosome are considerably more numerous than the exceptional daughters which arise from the fertilization of eggs containing two X-chromosomes. Since the effect of X-rays is to increase proportionately the numbers of exceptional sons and daughters, the increased number of exceptional daughters obtained as a result of X-raying their mothers is considerably less than the increased number of exceptional sons. Up to the time of publication of the notes to which reference is made above, the data obtained showed clearly that X-rays caused an increased production of exceptional sons and probably also of exceptional daughters. Since that time the technique[2] has been improved and a large number of experiments have been performed. The data now accumulated make it reasonably certain that X-rays not only cause non-disjunction but that the exceptional daughters of X-rayed mothers are in most cases fertile and themselves produce exceptional offspring. Reference will here be confined to the third and fourth series of experiments which were performed with the improved technique. The results of the third series were stated at the Boston meeting of the American Association. The fourth series of experiments have only recently been completed.

The improvements in technique involved, among other things, using for the X-rayed mothers virgin heterozygous flies resulting from crossing white-eyed long-winged females with eosin-eyed, miniature-winged males. This made it possible to test for the presence of exceptional females among the flies to be X-rayed. After X-raying, the females were mated to wild-type males. The use for X-raying of females heterozygous for characters located in the sex chromosomes made it possible to follow individually by the characters of the offspring the sex chromosomes which had been submitted to X-rays. In the third series of experiments 76 females were X-rayed and 79 females were kept as controls. In each experiment the X-rayed and control females were sisters. The X-rayed mothers produced a total of 1557 regular sons, 42 exceptional sons, 1771 regular daughters and 8 exceptional daughters. Seven out of eight exceptional daughters were fertile

[6] Acad. Roy. de Belg. *Bull.* Class. sc., 1905, p. 201-211.

[1] "On the elimination of the X-chromosome from the egg of Drosophila melanogaster by X-rays," SCIENCE, Vol. LIV, pages 277-279, September 23, 1921; "The production of non-disjunction by X-rays," SCIENCE, Vol. LV, pages 295-297.

[2] The technique of these experiments is described in detail in a paper submitted to the editors of the *Journal for Experimental Zoology.*

and produced further exceptions. The control mothers, sisters of above, produced a total of 7,531 regular sons, 5 exceptional sons, 7,711 regular daughters and one exceptional daughter. The probability that the difference between the offspring produced by the X-rayed and control mothers was due to chance can best be calculated by a formula of K. Pearson[3] for the probable error of a difference, for reference to which the writer is indebted to Professor Pearl. Calculation shows that in the case of the exceptional daughters the difference between the number produced by the X-rayed mothers (8) and that which might be expected from the behavior of the control (mean = .46023) is 13.10 times the probable error of the difference, (.50751). The same quantity for the sons is 49.81 times the probable error of the difference. These numbers make it practically certain that the difference is not due to chance. Every cause for the difference except X-rays seems to have been eliminated from the experiments—the control and X-rayed females were sisters emerging at approximately the same time, were kept at the same temperature, etc.

In the fourth series of experiments all the mothers, both those which were X-rayed and those in the controls, were the daughters of one white-eyed female by two eosin-eyed, miniature-winged males. The 26 X-rayed females produced a total of 1,934 regular sons, 42 exceptional sons, 2,173 regular daughters and 8 exceptional daughters. Seven of the eight exceptional daughters were fertile and produced exceptional offspring. The 19 control females produced a total of 5,109 regular sons, three exceptional sons, 4,985 regular daughters and one exceptional daughter. The difference between the number of exceptional daughters produced by the X-rayed females and those produced by the control females is 9.425 times the probable error of the difference. The same quantity for the exceptional sons is 41.00 times the probable error of the difference. The fourth series of experiments therefore confirms the findings of the third series. The results of the two series of experiments may be added together. In this case we find that the difference between the number of exceptional daughters (16) produced by the X-rayed females and the expected mean (.9354) is 20.17 times the probable error of the difference.

The exceptional daughters of the X-rayed females were of two kinds. Four of those in the third series were homozygous for both of the sex linked characters and were therefore formed by equational non-disjunction. Three of those in the third series and seven of those in the fourth series were heterozygous and therefore formed by reductional non-disjunction. Two of the exceptional daughters not included in the above two kinds were homozygous for one sex linked character and heterozygous for the other. Their constitution and the explanation of their occurrence will be discussed at a future date. It is clear from the results of the experiments described that X-rays may cause non-disjunction during either the first or the second of the maturation divisions and that the exceptional condition of the females produced by it, consisting in the possession of two X chromosomes and one Y chromosome, is transmitted to a proportion of their offspring. There is therefore in this effect of X-rays on the germ cells a very clear case of an external agent which modifies the mechanism of inheritance in such a way that a permanent effect is produced on the germ cells.

JAMES W. MAVOR.

UNION COLLEGE,
SCHENECTADY, N. Y.

[3] On the influence of past experience on future expectation, *Phil. Mag.*, Sixth Series, Vol. XIII, page 365.

OKLAHOMA ACADEMY OF SCIENCE

THE eleventh annual meeting of the Oklahoma Academy of Science was held in Oklahoma City on February 9, and at the University of Oklahoma, Norman, on February 10. The following papers were read:

Responses of bruchus to modified environments: J. K. BREITENBECHER.

The Isleta Indians: ALBERT B. REAGAN.

Some birds of the Oklahoma Panhandle: R. C. TATE.

Additional evidence on the possibility of the redemption of the great plains from its semi-arid condition: J. B. THOBURN.

The possible contribution of the motion picture in the effective teaching of history: J. W. SHEPHERD.

Present-day objectives in physics: HOMER L. DODGE.

Present-day objectives in zoology: A. RICHARDS.

Observations on the removal of the semi-circular canals from baby chicks: L. B. NICE AND C. L. FURROW.

Buried mountain ranges in Oklahoma: CHARLES N. GOULD.

Sources and uses of direct current in high school laboratories: B. C. BROUSE.

An interesting problem in bird study: R. O. WHITENTON.

Nesting records for 1920 to 1922 at Norman, Oklahoma: MARGARET M. NICE.

The identification and ecology of an epiphytic fern included in the Oklahoma cryptogamic flora: M. M. WICKHAM.

Unilateral inheritance in bruchus: J. K. BREITENBECHER.

The acceleration of fission rate in paramœcium: DIXIE YOUNG.

Concentric corpuscles of the palatine tonsils: J. M. THURINGER.

A note on the migration of miriapoda: J. M. THURINGER.

The parasites of dogs and cats in Oklahoma: JOHN E. GUBERLET.

Observations on the egg laying of the cow pea weevil: ALFRED BRAUER. Introduced by J. K. Breitenbecher.

The skeletal development of a 23-year old elephant compared with that of man: ED. CRABB.

The fall grasses of Cleveland County: C. W. PRIER.

Notes on the effect of urea on the diffusibility of MgSo4: ALMA J. NEILL.

Notes on biological survey: C. W. SHANNON.

A new geologic map of the Arbuckle Mountains of Oklahoma: C. E. DECKER.

A study of some conglomerates near the eastern limits of the redbeds of Oklahoma: F. O. EVANS.

Some phases of the Permian between the Arbuckle and Wichita Mountains, Oklahoma: E. R. BROCKWAY.

Phosphate rock in Oklahoma: A. C. SHEAD.

Chemical composition of Oklahoma sand barite rosettes: A. C. SHEAD.

The Osburn fault of northern Idaho: J. B. UMPLEBY.

An analysis of drillite and its bearing on the possible formation of igneous rocks from pre-existing sedimentaries: A. C. SHEAD.

Chemical composition of the Black Mesa: A. C. SHEAD.

Building materials of Oklahoma: M. C. OAKES.

An interpretation of certain peculiarities of the Canadian river basin of western Oklahoma: C. F. BOLLINGER.

Evidence of Permo-Pennsylvanian glaciation in the Wichita Mountains: S. WEIDMAN.

Outline of the structural geology of the Ouachita Mountains of Oklahoma: C. W. HONESS.

Notes on the paleontology of the Comanchean of Love County, Oklahoma: F. M. BULLARD.

Summarized data on the structural history of Oklahoma: BESS U. MILLS.

Some observations on the South Canadian River near Norman: O. F. EVANS.

An example of an Indian picture writing in the Wichita Mountains: S. WEIDMAN.

Present status of science work in Oklahoma: C. W. SHANNON.

Distribution of taxes in Oklahoma: F. F. BLACHLY.

The simple rigidity of a drawn tungsten wire at incandescent temperature: WILLIAM SCHRIEVER.

The cause of the optimum angle in a receiving conical horn: VICTOR A. HOERSCH.

An electrometer for measuring the radioactivity of gases from oil and gas wells: FORREST K. HARRIS.

A boy's vocabulary at eighteen months: GEORGE F. MILLER, MARGARET D. MILLER AND MARGARET M. NICE.

An experiment in automatic spelling: HERBERT PATTERSON.

The magnetism of the map: SOPHIE ALTSHILLER COURT.

Some problems in vocabulary testing: GEORGE F. MILLER.

A comparison of the sizes of the vocabularies of fifty children of the same age: MIRIAM OATMAN-BLACHLY.

A special feature of the program at the annual dinner, held on February 10, was the centennial celebration of the birth of Louis Pasteur and Gregor Mendel. The following program was presented:

Pasteur's contributions to chemistry: C. L. NICKOLLS.

Pasteur's contributions to bacteriology and pathology: GAYFREE ELLISON.

Louis Pasteur—the man: L. B. NICE.

Gregor Mendel: A. RICHARDS.

The following officers were elected for the ensuing year:

President, S. Weidman, Norman, Oklahoma; *first vice-president,* John E. Guberlet, Stillwater, Oklahoma; *second vice-president,* Frank G. Brooks, Oklahoma City, Oklahoma; *secretary,* L. B. Nice, Norman, Oklahoma; *treasurer,* J. K. Breitenbecher, Norman, Oklahoma; *curator,* Fred Bullard, Norman, Oklahoma.

L. B. NICE
Secretary

NORMAN, OKLAHOMA